GENERAL NOTES.

The Constant of Aberration.—The *Astronomical Journal* No. 571 contains an article by C. L. DOOLITTLE on "The Constant of Aberration." Professor DOOLITTLE gives the definitive results of eight series of observations for the determination of the constant of aberration, extending from 1889 to 1904. The first three series were made at the Sayre Observatory and the balance at the Flower Observatory. Altogether 15.363 pairs of stars were observed, and the final value deduced for the aberration constant is 20″.540. Professor DOOLITTLE remarks that no reasonable changes in the weighting of the results of the different series will alter this result more than 0″.01. One begins to wonder sometimes where the value of the aberration constant is going to stop. For a great many years STRUVE'S value, 20″.445, was used. In 1896 the Paris Conference adopted the value 20″.47 and Professor YOUNG gives this value in his work on General Astronomy with the remark that it is still uncertain by 0″.01 or 0″.02. In 1903 Dr. CHANDLER made an exhaustive investigation of this subject and expressed the conviction that the "real value of this much-disputed constant is likely to be found near or slightly above 20″.52."

And now, in 1905, Professor DOOLITTLE, as a result of more than fifteen thousand determinations of this constant, announces a value of 20″.54.

In the same number of the *Journal* mentioned above the committee on variable stars of the Astronomische Gesellschaft gives a list of fifty-eight new variable stars to which definitive designations have been assigned.

The *Astrophysical Journal* for March contains two interesting articles by Professor GEO. E. HALE, entitled, "A Study of the Conditions for Solar Research at Mount Wilson, California," and "The Solar Observatory of the Carnegie Institution at Washington." The latter article is illustrated by some excellent views.

The following notes have been taken from recent numbers of *Science:*—

"The Astronomical Observatory built by the late Dr. HENRY DRAPER at Hastings-on-Hudson in 1860 and used by him for his researches until his death in 1882, was destroyed by fire on March 31st. The telescopes and other instruments were removed to Harvard University in 1886, where, under the direction of Professor E. C. PICKERING, Mrs. HENRY DRAPER established the Draper Memorial Fund, but photographic negatives and other material of historic interest have been destroyed."

"A teaching observatory will be established by the Ontario government at the University of Toronto. Dr. C. A. CHANT expects to visit the observatories of the United States to study their plans and mehods."

Mr. PERCIVAL LOWELL has established a liberally endowed fellowship, to be known as the Lawrence Fellowship for the Department of Astronomy at Indiana University. By the terms of the endowment the Fellow is appointed by the department, but the appointment is subject to the approval of the founder. A Lawrence Fellow shall be given an opportunity for astronomical research at Lowell Observatory, and to prepare a thesis on some astronomical subject agreeable to the Director, and the Fellow Mr. JOHN C. DUNCAN, '05, has received the appointment for 1905-1906.

The Fifth Satellite of Jupiter.—In number 100 of these *Publications* Professor BARNARD published a reply to my criticism (*A. S. P.*, No. 98) of Miss DOBBIN'S computation of the orbit of the fifth satellite of *Jupiter.*

It seems almost incomprehensible that a computer, who has the best reason in the world for not including other observations,—namely that there were no other observations of a similar kind,—should fail to state that fact, and should instead try to justify the course pursued, of using only one person's observations, on such indefinite grounds as gaining "homogeneity," avoiding "personality of different observers," etc. It should be the aim of the computer to

eliminate rather than to *avoid* personalities of the observers,—or systematic *errors,* as I prefer to call them. That the systematic errors of competent observers in measuring the position-angle and distance of a satellite could be of such magnitude as to mask the quantities sought seems to me highly improbable, and neither Professor BARNARD nor Miss DOBBIN has shown in any way that this could be so. I for one am unwilling to believe without proof that such could be the case.

Professor BARNARD objects to my citation of Mr. HINKS's determination of the solar parallax. That the cases are not parallel is readily admitted. I had in mind in particular Mr. HINKS's remarks near the bottom of page 726 (*M. N.,* June, 1904), where he states: " . . . while the quite unexpected large errors in the Algiers plates, *taken with a refractor of standard pattern, cannot fail to inspire many stimulating doubts as to the absolute value of results obtained with one instrument alone.* At the same time elimination of the larger part of the systematic errors, which seems to have been achieved, assures us at once of the practicability of making a general solution, and of the difficulty of treating the results of any one observatory apart from the others." (Italics mine.) These words were written concerning a specific problem, and that they do not apply to all problems goes without saying. I gave this as an *illustration* of the benefit to be derived from comparing, for the purpose of detecting and eliminating systematic errors, observations made at different places.

Professor BARNARD cites Dr. CHANDLER'S determination of the orbits of the companions to Comet V 1889 as an illustration of the case of using the observations of only one man. A brief statement is as follows, the quotations being from Dr. CHANDLER'S article (*A. J.,* Nos. 236-237) :—

"The companion *C* was by far the most continuously and generally observed, and indeed the only one in which there are adequate means for the determination of a satisfactory orbit. With a few exclusions for mistakes or incompleteness, we have 155 positions, covering an interval of 114 days, contributed by sixteen observatories. Of these the Lick series, by BARNARD, constitutes over one third, begins earlier, ends later, and is more continuous than any other. It has a

superior degree of excellence as to smallness of accidental error; and notwithstanding some small peculiarities that will appear later in the discussion, we may be warranted in assuming that the advantages of exceptional atmosphere and aperture reduce to a minimum the chance for varying systematic error, dependent on the changing aspect of this faint object during its long season of visibility. I have, therefore, unhesitatingly chosen this series as a zero of reference for the constant errors of the other observations, and have also given an independent solution based on the Lick observations alone, *for comparison, with, and re-enforcement of,* the conclusions drawn from the general solution." (Italics mine.) It is seen from this that the other observations were not rejected, and the solution from BARNARD'S observations was merely used to re-enforce the conclusions drawn from the general solution. As far as I am able to find, Dr. CHANDLER makes no statement concerning the relative merits of the results of the two solutions. Instead of being an example of "one-man" work, Dr. CHANDLER'S article seems to me to be a most excellent example of the combination of observations made at different observatories.

For the companion *B* there were twenty-three observations made at the Lick Observatory and six at Vienna. Concerning these Dr. CHANDLER says: "In attempting to find the most probable orbit of *B* from a discussion of the above material, we meet two obstacles to a satisfactory solution. The first arises from the serious discordances between the Vienna and the Lick observations, already noticed in a less degree in those of companion *C* (p. 157). . . . After expending much time and labor in futile experiment with various hypotheses, I am forced to the conclusion that the only way to meet the first difficulty—since the Vienna observations are not numerous enough to be independently discussed—is to assume the correctness and homogeneity of the Lick series, with the 36-inch, and to base the calculations on them alone."

I am indeed surprised that Professor BARNARD should quote this instance to uphold his procedure. There is as much difference between the two cases as there is between day and night. Dr. CHANDLER rejected the Vienna observations only after spending much time and labor in an attempt to reconcile the two series, while Professor BARNARD would reject to

begin with all other observations for fear of confusion that may arise from systematic errors. It should go without saying that no general conclusions can be drawn from Dr. CHANDLER'S procedure in this case.

Professor BARNARD cites a second case to uphold his method. It is the determination of corrections to the elements of the orbit of the satellite of *Neptune,* by Professor ASAPH HALL (*A. J.,* 441). Professor HALL computed the orbit of this satellite in 1883, and later chose a series of observations made in 1897 and 1898 by Professor BARNARD, with the 40-inch Yerkes telescope, to test the accuracy of the elements. No statement is made by Professor HALL of the reasons he may have had for not including other observations. No other series contains anywhere near as many observations as BARNARD'S, and it may be that Professor HALL considered all others as sporadic. If so, that would be sufficient ground for rejecting them, but others might legitimately differ with him as to the sporadicalness of the observations. Or again, he may have considered that the time was not ripe for making a definitive discussion of the orbit, or it may be that he did not care to enter into the amount of labor that a complete discussion would call for. My own opinion is that Professor HALL'S main object was to test the orbit determined by him and this could be done by means of one series of observations as well as by many, and it was, perhaps, not incumbent upon him to state the reasons for proceeding as he did. It is not always necessary for a person to state the reasons why he has not done something which he might have done; but if he does, the reasons should be good ones.

In writing the criticism I had no intention whatever of casting reflection or suspicion upon Professor BARNARD'S observations. It has been shown a number of times that he makes micrometric measures of a high degree of excellence and that his observations are in general particularly free from both accidental and systematic errors. My criticism of Miss DOBBIN'S work is solely that the reasons assigned for her procedure are faulty and insufficient,—although, as a matter of fact, the best of reasons did actually exist, and I have every reason to believe that she has done a very creditable piece of work.

S. D. T.